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A meta-regression analysis on intergenerational transmission of education: publication bias and genuine empirical effect.[a]

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Abstract

In this article, we evaluate to what extent parental education impacts the education of their children by using a meta-regression analysis. Since the mi-1970s, there is a large and growing literature that deals with the causal impact on parental education on children's education. Those studies exhibit a large range of values for the education transmission coefficient. We consider an alternative way to estimate a true effect of parent education, discussing the existing empirical literature by using a meta-regression analysis. Our database is composed of a large set of both published and unpublished papers written over the last 40 years (1974-2014). This database allows us to econometrically evaluate an effect of parents education on their children, irrespective of articles heterogeneity (data sources, included explanatory variables, econometric strategy, type of publication), and of publication bias. We find evidence for both a publication bias and a large transmission coefficient of education.

Key-words: education, intergenerational transmission, meta-regression analysis.
JEL Classification: C83, J13, J24.

1. Introduction: evaluating the causal impact of parents' education on children's one

In this article, we evaluate to what extent parents' education is transmitted to their children, considering an alternative to what is usually done in existing literature.
Since the mi-1970s, a wide strand of empirical studies aimed at evaluating the causal impact of parents' education on children's one. On the one hand, empirical studies show that the (raw) intergenerational *correlations* in education amount to about 0.4 for Western Europe, 0.46 for the United States, and 0.6 for South America (Black and Devereux, 2011). On the other hand, the *causal* estimates of parents' education on children's schooling usually exhibit a smaller value (Holmlund *et al.*, 2011). Understanding the underlining mechanisms that explain these correlations leads to the debate "nature *vs* nurture". This debate aims at explaining the individual accumulation of human capital, distinguishing the impact of genetics and the impact of environment in which the child grows (including parental background). In the literature, the search for causal effect of parental education on children's education corresponds to the study of the existence of a "nurture effect" (Lochner, 2008; Holmlund *et al.*, 2011). Typically, the

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coefficient of intergenerational transmission of education (*i.e.* from parents to children) depicts this causal effect. Three main estimation strategies were implemented in the recent literature to evaluate the "causal effect" of parental schooling (Black and Devereux, 2011): the use of sample data with twins, with adoptees or instrumental (IV) strategies. In addition, Holmlund *et al.* (2011) consider the main methods used in the literature (OLS or IV estimators, twins and adoptees data). They consider a unique dataset from Sweden, and then compare their results with those of existing literature. They conclude that intergenerational associations are largely driven by selection and that parental education represents the main part of the "parental effect" but does not play a large role as a whole. Two recent papers offer also quite different approaches. First, Maurin and McNally (2008) use a natural experiment different to the ones used in most of the IV approaches which exploit changes in the education regulation. Due to the massive social movement of *"Mai 68"*, the pass rate in 1968 for French students have been enlarged and has permitted to "more" students to have their year in the education system certified. The authors show notably that this has also had an intergenerational effect (the children for the concerned students show a surplus of educational performance). Second, de Haan (2011) uses also another type of approach on US data. She uses a nonparametric bound analysis and finds lower estimation of the effect of parental schooling on children's schooling than those obtained with OLS. This result is in line with twins and adoptees studies or some of the IV studies.
Yet, there is an always on-going debate on the causal effect of parental schooling. Among recent studies, there is no consensus on this effect. For instance: Black, Devereux and Salvanes (2005) found little causal impact of parental education with IV (exception: for mothers-sons) while Pronzato (2012) underlines a quite strong effect of father's education (similar in that to previous twins' studies), but also a positive (and smaller) effect, of mother's education. Whatsoever, its real size remains unknown because of heterogeneous results that were found. Our study considers an alternative approach to what is usually done in the existing literature. We apply a meta-regression analysis (MRA hereafter) to a large set of empirical studies (1974-2014) that deal with evaluation of the transmission of human capital from parents to their children. Our aim is to get a value of the "true" effect of parents' education on children's one, *i.e.* while taking account of heterogeneity of empirical studies and of publication bias (Stanley and Jarrell, 1989; Stanley, 2005). On the one hand, empirical studies are characterized by many features (considered population group, included explanatory variables, econometric strategy, data sources, and type of publication). They may explain why results coming from various studies differ (Stanley and Jarrell, 1989; Stanley, 2001). On the other hand, as mentioned in Begg and Berlin (1988) for medical studies, papers with positive results are more likely to be published than studies with negative results. More generally, published results may overstate or understate the true effect (Stanley and Jarrell, 1989; Ashenfelter *et al.*, 1999; Huang *et al.*, 2009).

We contribute to the existing literature at three levels. First, we show that empirical studies gave rise to a large range of values for the education transmission coefficient because of articles heterogeneity (population under study; included explanatory variables; econometric strategy, data sources, characteristics of publications). Second, we test for publication bias in the literature dealing with the causal impact of parental schooling on children's schooling. In particular, we show existing results tend to overstate the true effect. Third, our article shows evidence of a genuine empirical effect of parental schooling on children's schooling, net of the publication bias and of the heterogeneity of the studies. This effect is more intense if we consider fathers than mothers for any child, or parents and children of the same gender (mothers and daughters, or fathers and sons).
The remaining of the paper is organized as follows. Section 2 presents the strategy and the meta-analysis regression dataset. Section 3 provides first results for publication bias and an empirical genuine effect of parents' education. Section 4 explores heterogeneity in reported

results of effect size to take account of characteristics in considered studies; it also analyses the heterogeneity in the estimated coefficient of intergenerational transmission coefficient, considering different kinds of parents (mothers/fathers) or of children (girls/boys). Section 5 concludes.

2. Meta-regression analysis: a dataset of education transmission coefficient estimates

In this Section, we display the empirical framework and present the data set we build to lead our meta-regression analysis[d]. We then explore a first simple 'size effect' approach.

2.1. Framework

In the empirical literature that deals with estimation of individual human capital accumulation (Mulligan, 1997 for instance), the education attainment of a child is explained by a large set of individual, familial as well as by other environmental variables. The following equation is thus estimated:

$$EDU_c = \alpha + \beta EDU_p + \gamma X + \varepsilon \quad (1)$$

where EDU_c is the education attainment of the given child, whereas EDU_p represents that of of parents. X is a set of control variables: they either refer to individual (gender or date of birth, for instance), or familial (income of parents, rank among siblings or education of grandparents for instance) features. They can also correspond to geographical variables. ε is the standard residual.
β is the coefficient of interest. It refers to the transmission of education from parents to their children. In this study, we focus on this coefficient that corresponds in many studies to what is called the causal effect of parents' education on that of their children.

2.2. Studies included in the MRA dataset

All empirical works that estimate an intergenerational coefficient of transmission of education are candidate to be included in the meta-regression analysis. To collect the set of studies to be included, we consider the works that deal with an evaluation of the causal impact of parental education on children's education. We also consider other studies that estimate 'human capital accumulation' functions with or without covariates including (at least) parental education. The included studies should explicitly report the value of the effect of parental education on children's education. Parents' and children's education level should be expressed in years of schooling. Indeed, completed years of schooling are considered in a large part of empirical studies that deal with the individual transmission of education.[e] The coefficient of intergenerational transmission should not correspond to an elasticity: the main part of this literature does not express the years of education in logarithm. Finally, studies where the education variable is the level of diploma cannot be included in our database because the

[d] See Stanley *et al.* (2013) for guidelines for this task.
[e] Alternative measurements of education include highest level of diploma achieved by the individual and will be considered in further research.

econometric results in these ordered logit/probit models cannot be directly compared to those obtained in linear models (years of schooling).

We performed several searches on scholar databases, and internet searches over December 2013 - February 2014, using a large set of keywords that are closely related to the specific subject of impact of parental education in the human capital literature.[f] First, we performed searches on EconLit databases (Cairn, JSTOR, Science Direct and Springer Link) for academic published papers. Second, we extended the search for working papers or research reports, on websites of specialized research institutions on labor/education economics: IZA, NBER, and SSRN. Third, we made extended research on a large number of google pages' results to include papers that may still be in progress, and other non-published research. Finally, we investigate that no work was forgotten by searching in the references in the selected papers. If different versions of the same paper exist, we consider the published version of the paper, or the most recent version of the article if it is still unpublished in any academic journals, conference proceeding or other book. We consider only papers with cross-section data, *i.e.* where there the individual observations concern a quite large range of birth cohorts, such as samples representative of a population or other more specific samples (twins or adopted children), but not for only one or a few numbers of cohorts. The final dataset was checked for possible errors in coding of the different variables and insure coherence into it.

2.3. Effect size

Our final dataset contains information provided by 65 articles published or written over 1974-2014. This set of 889 estimates for the education transmission coefficient corresponds to the effect size (Stanley and Jarrell, 1989). A given effect size corresponds to the estimate of the intergenerational transmission of education from parents to their children while estimating equation (1).

Table 1 lists the articles included in our file drawer. On average, a given study was written (or published) in the mid-2000's, and the average year of the sample data used in the studies is 1990. We also find on average 14 estimated values for the effect size in each study. Finally, the average coefficient found in the empirical studies is 0.23.

[f] These expressions are the following: *intra-family transmission of education, intergenerational transmission of education, educational intergenerational mobility, intergenerational education/schooling mobility, educational persistence, correlation between parents and child's schooling or education, intergenerational education correlation, intergenerational effects, intergenerational associations/transmissions, causal effect of parent's schooling on child's schooling, intergenerational schooling associations, transmission of human capital/education, causal relationship between parents' and children's education, and accumulation of human capital.*

Table 1. Studies included in the meta-regression analysis.

Author(s)	Average year of the survey	No. of effect sizes in study	Average effect size
Aguero and Ramachandran (2010	2002	4	0.084
Akbulut and Turan (2013)	1999.25	12	0.282
Alwin and Thornton (1984)	1981	1	0.231
Amin, Lundborg and Rooth (2007)	2007	56	0.140
Anger and Haneck (2010)	2006	2	0.298
Antonovics and Goldberger (2005)	1987	8	0.279
Ashenfelter, Collins and Yoon (2005)	1950	2	0.342
Assaad and Saleh (2013)	2010	8	0.255
Behrman and Rosenzweig (2002)	1987	24	0.135
Behrman and Rosenzweig (2004)	1990	4	0.145
Behrman and Taubman (1985)	1979	36	0.148
Belzil and Hansen (2003)	1979	8	0.206
Bevis and Barrett (2013)	1994	4	0.313
Bingley; Christensen and Jensen (2009)	1956	48	0.139
Bjorklund, Janti and Solon (2007)	1999	48	0.125
Björklund, Lindahl and Plug (2004)	1999	24	0.148
Björklund, Lindahl and Plug (2006)	1999	12	0.130
Black, Devereux and Salvanes (2005)	2000	12	0.156
Bruck and Esenaliev (2014)	1997.6	13	0.207
Case, Lin and Mc Lanahan (2001)	1976.5	3	0.081
Couch and Dunn (1997)	1986.5	8	0.253
Daouli, Demoussis and Giannakopoulos (2010)	2004.5	2	0.281
Datcher (1982)	1978	4	0.041
Davis (1994)	1980.5	4	0.147
De Haan (2008)	1989.5	32	0.261
De Haan and Plug (2009)	1989.5	16	0.341
Dearden, Machin and Reed (1997)	1991	6	0.328
Dumas and Lambert (2005)	2003	4	0.267
Duncan (1994)	1979.5	8	0.185
Duncan, Kalil, Telle and Ziol-Guest (2012)	1988.5	2	0.21
Emran and Shilpi (2012)	1999.5	18	0.520
Emran and Sun (2012)	2002	72	0.185
Ermisch and Proonzato (2010)	1997	8	0.154
Estudillo, Quisumbing and Otsuka (2001)	1993	4	0.07
Farré, Klein and Vella (2012)	1979	8	0.094
Fess, Moosr and Schuerz (2009)	2008	2	0.67
Hardy and Gershenson (2013)	1979	2	0.33
Havari and Savegnago (2013)	2005	8	0.406
Hertz, Meurs and Selcuk (2009)	1998	4	0.433
Hill and Duncan (1987)	1982	8	0.12
Hoffman (2013)	2000.5	12	0.053
Holmes (2003)	1991	8	0.188
Holmlund (2006)	1950	6	0.403
Holmlund, Lindhal and Plug (2011)	2006	28	0.143
Kahanec and Yuksel (2010)	2004	5	0.244
Kallioniemi (2014)	2004	8	0.168
Krein and Beller (1988)	1973.5	16	0.145
Kremer (1997)	1978	8	0.07
Kuo and Hauser (1995)	1973	16	0.221
Labar (2007)	1997.5	3	0.233
Leibowitz (1974)	1960	12	0.129
Lindahl, Palme, Massih and Sjögren (2013)	1997	4	0.287
Meng and Zhao (2013)	2005	20	0.294
Nimubona and Vencatachellum (2007)	1997	22	0.221
Pena (2011)	2001	81	0.504
Plug (2004)	1974.5	12	0.27
Plug and Vijverberg (2005)	1974.5	12	0.203
Pronzato (2012)	1997	6	0.162
Sacerdote (2000)	1979	4	0.253
Sacerdote (2004)	1975	6	0.145
Sacerdote (2007)	1975	3	0.167
Schultz (2004)	1987.75	8	0.343
Stella (2005)	2005	10	0.358
Tsou, Liu and Hammitt (2012)	2006	36	0.094
Wolfe, Haveman, Ginther and An (1996)	1988	2	0.445
Sample average	**1989.70**	**13.8**	**0.228**

Sources: Authors'compilation. See Appendix for full references.

3. Filtering publication selection bias from education transmission research: a first approach

In this section, we provide some evidence for a potential publication bias and a genuine empirical effect for the education transmission coefficient (heterogeneity of studies not taken into account). In particular, as usually done in meta-regression analyses (Doucouliagos and Jarrell, 2009; Huang *et al.*, 2009; Stanley, 2005), we disentangle publication bias and genuine empirical effect using funnel asymmetry and precision effect testing (FAT-PET).

3.1 Publication bias: funnel asymmetry

Funnel asymmetry testing

The literature that deals with meta-regression analysis and tries to estimate a genuine effect often distinguishes the true effect and publication bias. Indeed, Begg and Berlin (1988) showed for medical studies that papers with positive results (*i.e.* indicating a positive effect of the 'treatment') are more likely to be published than other. More generally, more particularly in economics, published results may overstate or understate the true effect (Stanley and Jarrell, 1989; Huang *et al.*, 2009). The result is that estimated effects of parental schooling may be correlated with sampling errors. If they are correlated with other variables, conclusions about the determinants of children's schooling may be seriously biased. The existence of any such bias is the natural working of a scientific process designed to discover important new results (Ashenfelter *et al.*, 1999).

Funnel plot is a first approach to detect publication bias (Sutton *et al.*, 2000; Stanley, 2005). For all studies in the MRA dataset, it displays an empirical relationship between the estimated beta coefficient and its precision (usually the inverse of standard error estimate). As mentioned in Sutton *et al.* (2000), an overweight plot on one side or another around the 'true effect' of parental education should be the sign of the existence of any publication selection. Thus, we first perform funnel plots on the whole sample. Then, we consider three sub-samples, depending on the kind of publications: academic publications, other publications and unpublished papers. The top plot of Figure 1a shows there may exist some publication bias: considering the whole sample leads to an overweight on the right side. It seems to be due to what happens with "other published papers" or the "unpublished papers" (Figure 1b); it is less clear when only considering "academic papers", where symmetry occurs (bottom of Figure 1a).

However, funnel plots are only graphs. We can perform a formal test for the funnel graph's asymmetry (Stanley, 2005). The starting point for Funnel Asymmetry Testing (FAT hereafter) is the relationship between the reported coefficient of parental transmission of education and its standard error (Egger *et al.*, 1997):

$$\beta_j = \beta_1 + \beta_0 SE_j + u_j \tag{2}$$

β_j corresponds to the estimated transmission coefficient of education from parents to their children. It is reported in the j^{th} study of our final dataset ($j = 1, 2, \ldots N$). SE_j is the standard error of β_j, and u_j is a random residual. If there is no publication bias, the estimated effects should randomly vary around the genuine value of the coefficient β_1. Since the research studies in economics use different sample sizes and different econometric models and techniques, u_j

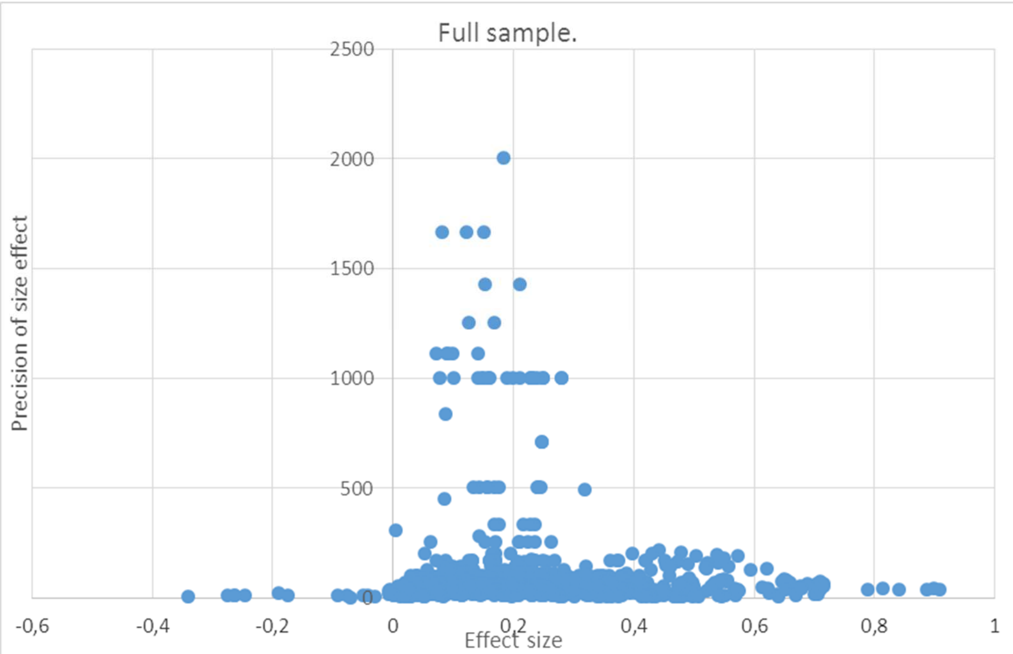
are likely to be heteroscedastic. To cope with this problem, we apply OLS on equation (2) where all terms are divided by SE_j:

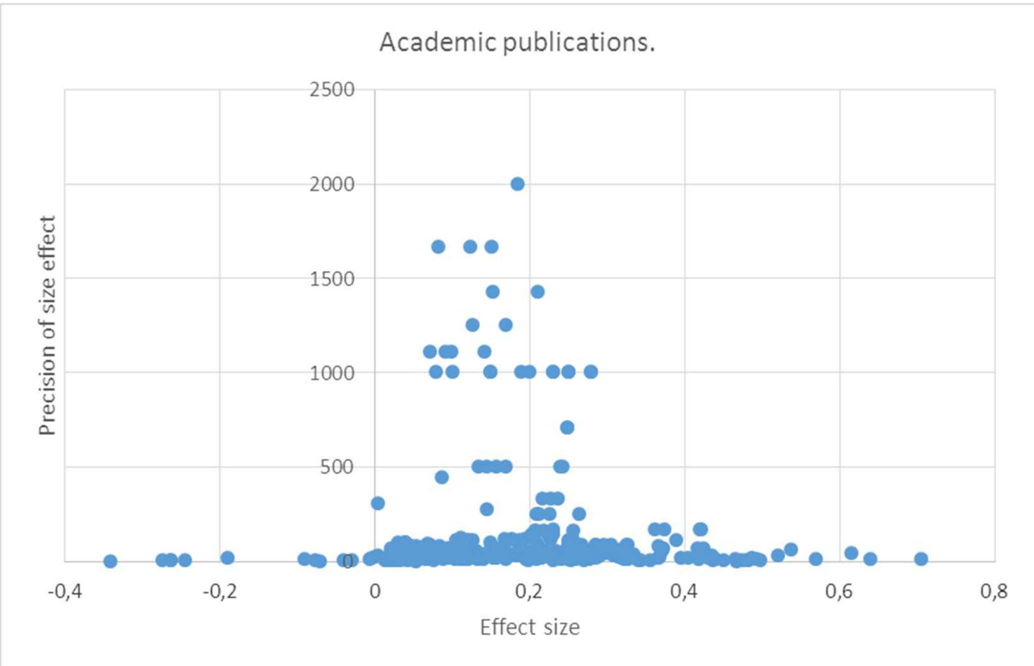
$$t_j = \beta_0 + \beta_1 \frac{1}{SE_j} + v_j \tag{3}$$

where t_j refers to t-value associated to β_j.

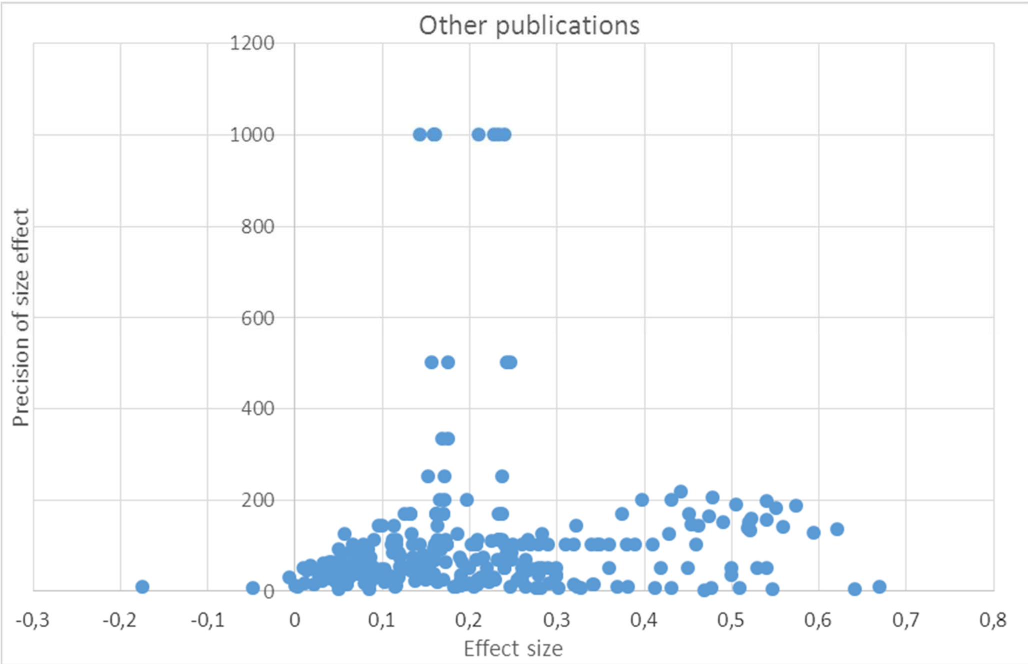
The Funnel Asymmetry Testing (FAT hereafter) consists in a *t*-test performed on the intercept (β_0). If β_0 is different from zero, there is evidence for funnel symmetry and thus for publication bias.

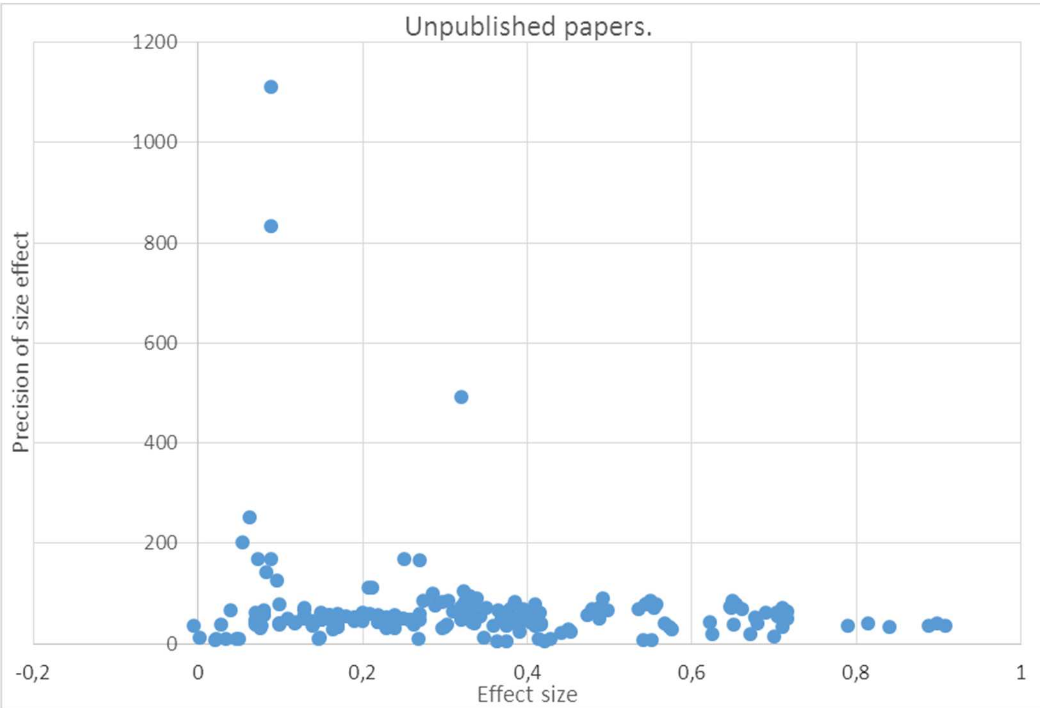
Figures 1a. Funnel plots for the intergenerational transmission of education, different sub-samples of observations.





Figures 1b. Funnel plots for the intergenerational transmission of education, different sub-samples of observations (continued).





In our dataset, a lot of studies report more than one and sometimes a large number of estimated values for the beta coefficient. We thus have to take it into account by clustering standard errors. Otherwise, FAT is known to be biased (Sterne *et al.*, 2000; Macaskill *et al.*, 2001). Indeed, the precision indicator $(1/SE_j)$ includes random sampling errors because it must be estimated. Hence, we also perform estimations of equation (3) by using the funnel asymmetry instrumental variables estimator (FAIVEHR) that allows full heteroscedasticity-robust estimations (Davidson and MacKinnon, 2004). To proceed, we use the square root of the number of estimates within the whole dataset as an instrument for $1/SE_j$ [g].

FAT results reported in Table 2 confirm what has been found through shown by the funnel plots. First, considering the whole dataset and whatever the estimator we consider (clustering or not standard errors), FAT exhibits a positive publication bias. The coefficient of the intercept is 4. It means that the estimated education intergenerational transmission coefficient tends to overstate the true effect (β_1). However, among the three subs-samples, this result is of smaller importance considering only "academic publications", even if where β_0 is always significant[h].

Table 2. Funnel Asymmetry and Precision Effect Testing.

Moderator variable	**OLS**			**IV-FAIVEHR**		
	None	Robust	Cluster	None	Robust	Cluster
	Full sample					
Intercept	4.318***	4.318***	4.318**	4.195***	4. 195***	4. 195*
	(0.685)	(0.705)	(2.123)	(0.712)	(0.900)	(2.242)
1/Se	0.166***	0.166***	0.166***	0.167***	0.166***	0.166***
	(0.003)	(0.009)	(0.017)	(0.003)	(0.011)	(0.021)
Sample size: number of estimates	874	874	874	874	874	874
R^2	0.809	0.809	0.809	0.851	0.851	0.851
	Academic publications					
Intercept	2.048*	2.048***	2.048**	2.442**	2.442**	2.442*
	(1.146)	(0.751)	(0.967)	(1.173)	(0.989)	(1.267)
1/Se	0.162***	0.162***	0.162***	0.159***	0.159***	0.159***
	(0.003)	(0.011)	(0.018)	(0.004)	(0.012)	(0.020)
Sample size: number of estimates	371	371	371	371	371	371
R^2	0.856	0.856	0.856	0.851	0.851	0.851
	Other publications					
Intercept	2.002*	2.002*	2.002	-0.667	-0.667	-0.667
	(1.093)	(1.112)	(3.113)	(1.285)	(1.038)	(2.302)
1/Se	0.203***	0.203***	0.203**	0.229***	0.229***	0.229***
	(0.006)	(0.012)	(0.014)	(0.008)	(0.015)	(0.036)
Sample size: number of estimates	304	304	304	304	304	304
R^2	0.811	0.811	0.811	0.852	0.852	0.852
	Unpublished papers					
Intercept	11.085***	11.085***	11.085**	5.466***	5.465	5.465
	(1.168)	(0.705)	(4.936)	(1.810)	(3.774)	(6.657)
1/Se	0.108***	0.108***	0.108***	0.194***	0.194***	0.194
	(0.010)	(0.026)	(0.034)	(0.021)	(0.069)	(0.126)
Sample size: number of estimates	199	199	199	199	199	199
R^2	0.392	0.392	0.392	0.583	0.583	0.583

Notes: standard error within parentheses are computed without any correction, to be robust or clustered. Cluster is equal to the number of estimates in a given study. FAIVEHR refers to funnel asymmetry instrumental variables estimator; sqrt(sample size) is the considered instrumental variable. *** (resp. ** and *) stands for significance at a 1% (resp. 5% or 10%) level.

3.2. Empirical genuine effect: precision effect testing and meta-significance testing

The most important scientific question concerns whether there is an underlying genuine empirical effect, irrespective of publication selection.

[g] For statistical reasons, this variable should be highly correlated with $1/SE_j$ (Stanley, 2005).

[h] Note that this does not imply there is no publication bias considering other publications or unpublished papers. Indeed, FAT has been proved to have a low power (Egger *et al.*, 1997). However, even if we employ a more 'liberal' significance level (10% for instance), our conclusion still remains unchanged.

Besides testing for publication bias, equation (3) offers the possibility to test for a genuine empirical effect (Stanley, 2005). Indeed, β_1 refers to the "true" effect (or "empirical genuine effect"). Thus, a significant and positive β_1 indicates a positive effect of parental education net of publication bias. This is the Precision Effect Test (PET).
Table 2 shows that β_1 is always significant. There is evidence for a genuine empirical effect once we take account for publication bias. Moreover, $\hat{\beta}_1 \in [0.11; 0.23]$. Indeed, estimated true effect of parents' education on their children education using the full sample is of the same order as that one we get considering only "academic publications". On the contrary, estimated true effect is somehow greater for "other publications" or "unpublished studies". Nevertheless, such differences may not be significant.
Since PET has a low power, we can also use another method suggested by Stanley (2001) to test for an empirical genuine effect, while removing publication bias: the meta-significance testing (MST hereafter). MST observes the possibility of a genuine empirical effect even with the existence of potential publication bias. The method to remove potential publication bias is based on a property of statistical power: the magnitude of the standardized test statistics will vary positively with the sample size only if there is in fact an overall genuine empirical effect. Consider the following equation (Stanley, 2005):

$$\ln\left|t_j\right| = \alpha_0 + \alpha_1 \ln n_j + e_j \quad (4)$$

The α_1 coefficient should be equal to 0 if there is no empirical effect ($H_0 : \alpha_1 = 0$ is true). It should be equal to ½ if there exists any empirical effect (H_0 is rejected, Stanley, 2005).
Table 3 reports results of MST. α_1 is always significant, whatever the sample we consider. Thus, we cannot say there is no genuine empirical effect. Moreover, considering the full sample, the estimated value of the coefficient is equal to 0.47, and is thus close to 0.5. However, $\hat{\alpha}_1$ is far smaller than 0.5 considering "academic publications", whereas it is hardly greater than 0.5 considering only "other publications" or "unpublished papers".
However, as pointed out by Stanley (2005), "*it is more likely that publication selection will decrease α_1 to below ½ but allow it to remain greater than 0 if there is a genuine effect*". Since the value of estimated α_1 is below ½ for the 'academic publications' sub-sample and a little above ½ for the 'other publications' and 'unpublished papers' sub-samples, this would indicate that there is co-existence of both publication bias and genuine effect, with some possible variation in the genuine publication bias among the different sample.

Our results indicate the likelihood of both publication bias and genuine empirical evidence for transmission of parental human capital. So far we did not consider any kind of heterogeneity among the studies included in our dataset. However, empirical studies that aim to evaluate the education transmission coefficient have, quite commonly, different features. To get an empirical genuine effect, net of both publication bias and heterogeneity of studies, we need to proceed to multivariate MRA.

Table 3. Meta-significance testing.

Moderator variable	OLS		
	None	Robust	Cluster
	Full sample		
Intercept	-1.755***	-1.755***	-1.755***
	(0.198)	(0.218)	(0.462)
Ln(sample size)	0.470***	0.470***	0.470***
	(0.024)	(0.027)	(0.058)
Sample size: number of estimates	874	874	874
R^2	0.312	0.312	0.312
	Academic publications		
Intercept	-1.473***	-1.473***	-1.473***
	(0.031)	(0.039)	(0.060)
Ln(sample size)	0.420***	0.420***	0.420***
	(0.031)	(0.294)	(0.461)
Sample size: number of estimates	371	371	371
R^2	0.331	0.331	0.331
	Other publications		
Intercept	-2.421***	-2.421***	-2.421*
	(0.374)	(0.352)	(1.200)
Ln(sample size)	0.548***	0.548***	0.548***
	(0.044)	(0.040)	(0.132)
Sample size: number of estimates	304	304	304
R^2	0.336	0.336	0.336
	Unpublished papers		
Intercept	-1.812***	-1.812***	-1.812
	(0.623)	(0.634)	(1.268)
Ln(sample size)	0.505***	0.505***	0.505**
	(0.074)	(0.079)	(0.179)
Sample size: number of estimates	199	199	199
R^2	0.192	0.192	0.192

Notes: standard error within parentheses are computed without any correction, to be robust or clustered. Cluster is equal to the number of estimates in a given study. *** (resp. ** and *) stands for significance at a 1% (resp. 5% or 10%) level.

4. Exploring heterogeneity in reported studies: a multivariate meta-regression analysis

4.1 Effect size and heterogeneity of studies

In Section 2, we show that the considered set of studies display large range of values for the effect size (see Table 1). However, all those studies are characterized by several specific features. Any estimated coefficient of the parental transmission of education is provided for a specific estimation within a given study. In particular, this coefficient is related: to a specific survey, to a given population of children or of parents, to given set of control variables that were included while estimating the effect size to a given econometric estimator, to a given econometric estimator or to a particular kind of publication. A given estimated value of the education transmission coefficient may thus be linked to all those specific features.

Thus, in what follows, we propose a definition and coding of the moderator variables, *i.e.* variables that describes empirical studies and may that appear relevant to explain why effect size β_j may differ across empirical studies.

Those meta-independent variables belong to one of the seven groups of variables:

- *Data type*: contains general information about data sources (country the survey).
- *Children*: provides information for characteristics of children (twins or adoptees children; ethnic origins; boys or girls, for instance) considered to get β_j.

- *Parents*: provides general information on parents that are considered (mother or father for instance) to get β_j.
- *Socioeconomic control variables*: those variables indicate whether or not some control variable are included in the econometric specification to get a given β_j. Control variables include characteristics related to children (age, gender) or their family (household income, number of siblings for instance). Local indicators are also taken into account.
- *Estimator*: refers to the econometric methodology used in the research.
- *Publication characteristics:* allows characterizing any kind of publications: academic journals, working papers, book chapters or conference proceedings; research field (general vs. specific); research quality (5-years Social Science Impact Factor).

For all those variables, Table A1 in appendix provides definitions and sample statistics (means and standard deviations) considering the full set of publications within our meta-database.
For every moderator variable that corresponds to a dummy variable, Table 4 reports the mean difference between the effect sizes for the target group and the effect sizes for the remaining (reference) group of estimated effect size. For instance, relatively to any other groups of countries, the effect size is on average greater for estimates that rely on data related to Americans, but smaller for estimates that rely on data related to Europeans. As well, relatively to any other kind of people, effect size is smaller for twins or adopted children. It is also smaller for estimates where the given study control for any kind of socioeconomic variables (age, gender, household income) or where the study include in the econometric equation the education levels of both parents ("assortative"). Besides, effect sizes are on average greater for estimates using an OLS estimator or in unpublished articles. Hence, effect size varies along with the features of estimates in most cases. The next step of our analysis is to perform multivariate meta-regression to take account of moderator variables.

Table 4. Mean effect size by characteristics of the study and the population on the full sample of the meta-regression study

Variable name	Difference[a]	Significance[b]
Data type:		
Africa	0.022 (0.018)	0.145
America	0.087 (0.012)	0.001***
Asia	-0.026 (0.012)	0.037**
Europe	-0.079 (0.010)	0.001***
Children:		
Normal	0.123 (0.010)	0.001***
Twins	-0.109 (0.012)	0.001***
Adopted child	-0.095 (0.012)	0.001***
Own birth child	-0.014 (0.011)	0.221
Boy	-0.009 (0.013)	0.499
Girl	-0.006 (0.014)	0.657
All gender	0.011 (0.011)	0.320
Black	-0.037 (0.019)	0.055*
White	-0.025 (0.016)	0.110
No color	0.033 (0.013)	0.014**
Parents:		
Mother	-0.052 (0.011)	0.001***
Father	-0.056 (0.010)	0.001***
Both parents	0.186 (0.015)	0.001***
Biological mother	-0.023 (0.014)	0.096*
Biological father	-0.048 (0.012)	0.001***
Adoptive mother	-0.082 (0.019)	0.001***
Adoptive father	-0.077 (0.016)	0.001***

Socioeconomic control variables:		
Gender	-0.049 (0.011)	0.001***
Age/Birth	-0.018 (0.013)	0.146
Number of siblings	-0.024 (0.012)	0.041**
Rank among siblings	-0.077 (0.014)	0.001***
Ethnic	-0.027 (0.012)	0.024**
Assortative	-0.151 (0.011)	0.001***
Birth parents	0.014 (0.012)	0.262
Professional status	-0.033 (0.011)	0.004***
Income	-0.057 (0.010)	0.001***
Grandmother education	-0.047 (0.018)	0.014**
Grandfather education	-0.047 (0.018)	0.014**
Local	-0.027 (0.011)	0.016**
No covariates	0.156 (0.038)	0.001***
Estimator:		
OLS	-0.010 (0.013)	0.450
IV	0.067 (0.020)	0.001***
Within	-0.107 (0.022)	0.001***
Other	0.007 (0.015)	0.643
Publication characteristics:		
Academic	-0.064 (0.011)	0.001***
Other publication (wp)	-0.028 (0.011)	0.011**
Unpublished	0.126 (0.016)	0.001***
General economics	-0.107 (0.011)	0.001***
Labour or population economics	-0.014 (0.011)	0.188
Other fields	0.039 (0.020)	0.053*
Adjusted number of citations	-1.7e-7(4.30e-8)	0.001***
Social Science Impact Factor	-0.001 (0.000)	0.051**

Notes: [a] Difference refers to the mean difference between the effect sizes for the target group and the effect sizes for the remaining (reference) group.. [b] P-value (probability to reject the alternative hypothesis) for the statistical significance of the group difference. *** (resp. ** and *) stands for significance at a 1% (resp. 5% or 10%) level.

4.2 Multivariate meta-regression analysis, publication bias and genuine effect

This FAT-MRA approach generalizes the FAT-PET-MST analysis. It allows us to estimate the "true" effect of parental education on children's education, *i.e.* net of the heterogeneity of the studies and of publication bias. To proceed, we generalize equation (3) where we add moderator variables divided by the effect size standard error (Stanley, 2005):

$$t_j = \beta_0 + \beta_1 \left(1 / SE_j\right) + \sum_{k=1}^{K} \alpha_k \left(Z_{jk} / SE_j\right) + \varepsilon_j \qquad (5)$$

Z_{jk} are the K moderators variables or meta-independent variables (Stanley, 2001). β_1 represents the "true" value of the transmission coefficient, whereas β_0 refers to publication bias. Finally, ε_j is the meta-regression disturbance term.
Equation (5) is estimated by OLS, considering clustered standard errors at the study level.

Table 5 provides evidence for publication bias, even once we control for heterogeneity of empirical studies.
First, empirical effect size is largely explains through heterogeneity (data sources, included explanatory variables, econometric strategy, type of publication) among studies that aim to analyze the impact of parents' education on children's one. For instance, ceteris paribus, effect size is smaller for estimates that rely on Africans than for Americans. However, this is not true considering only the "academic publication" sample. For Europeans, relatively to Americans, effect size is found to be smaller considering the full set of estimates, whereas it is found to be

greater among “academic publications”. Considering children’s characteristics, whatever the publication sample we consider, effect size is significantly smaller for twins, adopted children and girls. As well, relatively to estimates that are related to both parents, effect size are systematically smaller for *mother* and *father* groups. As to socioeconomic control variables, effect size is smaller in studies where the rank of the child among siblings or the education of both parents are taken into account. Effect size is greater if professional status of parents or birth cohorts of children are accounted for only considering “academic publication”. Including no covariate in the estimated equation only impacts positively effect size for the “Full sample”. Otherwise, relatively to using a basic OLS estimator, effect size is greater if an IV estimator is considered. To take account for possible changes in the coefficient of intergenerational transmission of education along the years, we also include dummy variables for (average) years of the data. Many of them appear to be significant in the estimations: considering academic publication sample, the later groups of years (1998-2003 and 2004-2010) negatively impacts the effect size. Finally, considering only academic publications, we see that effect size is smaller in studies published in labour and population economics journals than in general economics journals.

Second, let us have a look at publication bias. Publication bias is of smaller size than without controlling for this heterogeneity. Moreover, publication bias is still greater among articles that are published in “academic journals” than in working papers or other unpublished papers.

Third, the most important scientific question concerns the size of genuine empirical effect, when considering the transmission of education from parents to their children. Table 5 shows that the true effect is 0.528 considering all studies, or 0.331 considering only “academic publications”. Thus, we still find a genuine empirical effect of parental education on their children’s one, even irrespective of publication selection and heterogeneity of studies. Finally, we have to mention that R-squared of both regressions are rather large (higher than 0.90): it indicates that our models explain the main part of the heterogeneity in estimated coefficient of transmission of education attainment.

4.3. Discussing the main results

The FAT-MRA regressions have confirmed evidence found in Section 3 (FAT-PET-MST) for a genuine empirical effect of parental schooling on their children’s one. This true effect is estimated to 0.33 for the “academic publication” sample. It is interestingly higher than what is estimated in a large number of recent empirical studies[i] (for instance: Black, Devereux and Salvanes, 2005; Pronzato, 2012). As shown in the FAT-MRA on both samples, a quite large number of moderators are significantly correlated to the estimated coefficient of parental transmission of education. Hence, the heterogeneity of studies also explains a large part of the variation of the coefficient of parental transmission of education in the empirical studies related. This result explained what could firstly seem surprising: the true effect found is significantly higher *despite* a *positive* publication bias, *because of* a large heterogeneity in the existing studies. This result also underlines the limits of the ‘traditional’ empirical approaches aiming at estimating the causal impact of parental education on children’s education, whatever the level of sophistication used for the statistical tools or the building of the database. It gives an important ex-post justification to the use of meta-regression analysis for the subject of the present study.

[i] Please note that it is also significantly higher than the mean effect size in the whole set of studies (0.23, as shown in Section 2).

Table 5. Multivariate Meta-Regression Analysis of the effect of parental education on children's education.

Moderator variable	Full Sample	Academic publications (2)
Intercept	2.039* (1.048)	2.737** (0.892)
Precision (1/Se)	0.528*** (0.054)	0.331*** (0.062)
Data type:		
Africa	-0.130* (0.067)	0.118 (0.061)
America	Ref.	Ref.
Asia	-0.022 (0.42)	0.176*** (0.059)
Europe	-0.091* (0.051)	0.123** (0.053)
Children:		
Normal	Ref.	Ref.
Twins	-0.134*** (0.046)	-0.131** (0.062)
Adopted child	-0.121*** (0.041)	-0.100*** (0.031)
Own birth child	-0.027 (0.031)	0.017* (0.010)
Boy	-0.026 (0.026)	-0.004 (0.023)
Girl	-0.057** (0.027)	-0.044* (0.024)
All gender	Ref.	Ref.
Black	-0.012 (0.063)	-0.003 (0.055)
White	0.011 (0.060)	0.081 (0.056)
No color	Ref.	Ref.
Parents:		
Mother	-0.162*** (0.045)	-0.098*** (0.036)
Father	-0.170*** (0.047)	-0.086** (0.035)
Both parents	Ref.	Ref.
Socioeconomic control variables:		
Gender	-0.039 (0.026)	-0.019 (0.024)
Age/Birth	0.044 (0.036)	0.112*** (0.040)
Number of siblings	-0.002 (0.029)	0.032 (0.039)
Rank among siblings	-0.120*** (0.035)	-0.119** (0.052)
Assortative	-0.074*** (0.005)	-0.070*** (0.005)
Birth parents	0.012 (0.038)	-0.096* (0.050)
Professional status	0.039 (0.046)	0.124*** (0.045)
Income	-0.033 (0.027)	-0.012 (0.025)
Local	-0.040 (0.025)	-0.139*** (0.037)
No covariate	0.102*** (0.032)	0.042 (0.057)
Estimator:		
OLS	Ref.	Ref.
IV	0.056* (0.033)	0.040** (0.018)
Within	-0.039 (0.037)	-0.044 (0.049)
Other	-0.083** (0.037)	-0.112*** (0.021)
Publication characteristics:		
Academic	Ref.	-
Other publication (wp)	-0.023 (0.027)	-
Unpublished	-0.137 (0.043)	-
General economics	-	Ref.
Labour or population economics	-	-0.138*** (0.040)
Other fields	-	0.044 (0.048)
Social Science Impact Factor	-	-0.037 (0.026)
Number of estimates	869	363
R^2	0.915	0.980

Notes: Dependent variable is the t-statistic (effect size related to effect standard error). WLS estimates with clustered standard errors are computed at the study level. Some publication variables (fields of research, number of citations and social science impact factors) are only available for journals (academic publications).

4.4. Heterogenenity in the transmission of education

Empirical studies that analyze the transmission of educational attainment often consider several kinds of samples, distinguishing in particular among parents or children gender. For instance, they sometimes estimate the intergenerational coefficient between the education level of mother or father and that of her child. They may also focus on the same relation, considering parents as a whole, but distinguishing between boys and girls. Thus, we expand our analysis to eight kinds of samples, considering parents and / or children gender.

Table 6. Intergenerational transmission coefficient of education. Following the considered parent (mother, father) or child (girl, boy).

Transmission of education	Full sample	Academic publications
Parents to their daughter	0.614*** (0.045)	0.313*** (0.060)
Parents to their son	0.522*** (0.039)	0.570*** (0.102)
Mother to her child	0.347*** (0.039)	0.169** (0.066)
Father to his child	0.385*** (0.029)	0.330*** (0.042)
Mother to her daughter	0.412*** (0.058)	0.201*** (0.035)
Mother to her son	0.317*** (0.045)	0.140 (0.123)
Father to his daughter	0.427*** (0.040)	0.124*** (0.029)
Father to his son	0.432*** (0.036)	0.385*** (0.053)

Notes: dependent variable is the t-statistic (effect size related to effect standard error). Within parentheses, clustered standard errors are computed at the study level. Meta-independent variables include data sources (dummies for country), information on children (twins, adoptees, ethnic origins); socio-economic variables (age, gender of children; household income, number of siblings or rank among siblings); econometric estimators; and publication features (field of research, kind of publication; research quality). Some publication variables (fields of research, number of citations and social science impact factors) are only available for journals (academic publications).

Table 6 displayed corresponding results. It exhibits two main results, whatever the sample we consider (full sample of publications, or only academic publications). First, the intergenerational transmission coefficient of education is larger if it refers to education transmission of father than of the ‘mother. Second, the transmission coefficient is larger if the considered parent and child are of the same gender.

5. Conclusion

In this article, we provide new evidence for the causal effect of parental education on children’s education. To proceed, we build a large set of empirical studies that link parental and children’s educations. We then conduct a meta-regression analysis to estimate a true effect of parents’ schooling on children’s one, irrespective of the heterogeneity among considered studies and of any publication bias. By performing publication bias testing and multivariate meta-regression with publication bias, we find evidence for of genuine empirical effect of parental schooling, net of publication bias and of the heterogeneity of the studies. We also find that the heterogeneity of the studies also explains a large part of the variation of estimated coefficient of parental transmission of education in the considered empirical studies. Finally, even considering heterogeneity of studies, some publication bias was found that indicates that on average effect size in such kind of papers overstate the true genuine effect. Considering “academic publications”, the true effect is 0.33. This relation is more intense if we consider fathers than mothers for any child, or parents and children of the same gender (mothers and daughters, or fathers and sons). Overall, we find a causal impact of parental education in itself (and not of only parental revenue, for instance) on their children's education. This important result for public policies: actions directly raising parental education would have a clear benefit for their children.

In our sample dataset, we only focus on studies considering years of schooling as measure of achieved education, mainly for ease in the comparison of results. Further research on MRA regarding the impact of parental education on children's education could include the studies that consider level of diploma as a measure of human capital.

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Appendix

Table A 1. Summary statistics of the moderator variables in the meta-regression analysis.

Variable name	Variable Description	Mean (Standard Deviation)
Meta-Dependent variable		
T-stat	= Student t-statistic associated to the effect size.	22.21 (1.42)
Meta-Independent variables		
Estimate's accuracy:		
Inverted squared error (ISE)	= Inverted standard error (effect size precision).	108.07 (7.70)
Data type:		
Africa	=1, if the survey deals with a country in Africa.	5.72 (0.79)
America	=1, if the survey deals with a country in America.	39.01 (1.65)
Asia	=1, if the survey deals with a country in Asia.	21.74 (1.40)
Europe	=1, if the survey deals with a country in Europe.	33.52 (1.60)
Children:		
Normal	=1, if the estimated coefficient is related to any type of children.	53.32 (1.69)
Twins	=1, if the estimated coefficient is only related to twins.	21.40 (1.39)
Adopted child	=1, if the estimated coefficient is related to an adopted birth child (adoptees data).	13.16 (1.14)
Own birth child	=1, if the estimated coefficient is only related to an adopted child (adoptees data).	12.13 (1.10)
Boy	=1, if the estimated coefficient is only related to boys.	25.74 (1.48)
Girl	=1, if the estimated coefficient is only related to girls.	24.94 (1.46)
All gender	=1, if the estimated coefficient is related to both genders.	49.31 (1.69)
Black	=1, if the estimated coefficient is only related to black people.	4.58 (0.71)
White	=1, if the estimated coefficient is only related to white people.	3.20 (0.60)
No color	=1, if the estimated coefficient is for any ethnic origin.	92.21 (0.91)
Parents:		
Mother	=1, if the estimated coefficient is related to the mother of the child.	42.33 (1.67)
Father	=1, if the estimated coefficient is related to the father of the child.	40.85 (1.66)
Both parents	=1, if the estimated coefficient is related to both parents.	16.82 (1.26)
Biological mother	=1, if the estimated coefficient is related to the biological mother (adoptees data).	8.00 (0.92)
Biological father	=1, if the estimated coefficient is related to the adoptive father (adoptees data).	7.78 (0.91)
Adoptive mother	=1, if the estimated coefficient is related to the adoptive mother (adoptees data).	5.83 (0.79)
Adoptive father	=1, if the estimated coefficient is related to the adoptive father (adoptees data).	5.49 (0.77)
Socioeconomic control variables:		
Gender	=1, if the gender is considered as a control variable.	41.30 (1.67)
Age/Birth	=1, if age or birth cohorts are considered as control variables.	71.62 (1.53)
Number of siblings	=1, if the number of siblings is considered as a control variable.	27.23 (1.51)
Rank among siblings	=1, if the rank of the individual among siblings is considered as a control variable.	8.81 (0.96)
Ethnic	=1, if ethnic origin is controlled for.	9.38 (0.99)
Assortative	=1, if assortative mating is controlled for.	54.78 (1.69)
Birth parents	=1, if dummies for parents' year of birth are included as explanatory variables.	38.33 (1.65)
Professional status	=1, if any test score for parents is included as an explanatory variable.	12.01 (1.10)
Income	=1, if information about the socio-professional status of parents is included.	21.28 (1.38)
Grandmother education	=1, if the education of the grandmother in included as an explanatory variable.	3.54 (0.63)
Grandfather education	=1, if the education of the grandfather in included as an explanatory variable.	3.55 (0.63)
Local	=1, if local dummies are included as control variables.	31.35 (1.57)
No covariates	=1, if no control variables are included.	3.43 (0.62)
Estimator:		
OLS	=1, if OLS estimator is considered.	65.79 (1.61)
IV	=1, if an IV estimator is considered (instrumenting parents' education).	16.59 (1.26)
Within	=1, if a Within estimator is considered (mainly for adoptees data)	7.78 (0.91)
Other	=1, if other estimators are considered (2SLS instrumenting any of the control variables; censored model à la Tobit; simultaneous equations or structural model).	9.84 (1.00)
Publication characteristics:		
Academic	=1, if the study is published in an education economics journal.	42.45 (1.67)
Other publication (wp)	=1, if the study is published in working papers, book chapters or proceedings.	34.78 (1.61)
Unpublished	=1, if the study is unpublished.	22.77 (1.42)
General economics	=1, if the study is published in a general economics journal.	49.77 (1.69)
Labour or population economics	=1, if the study is published in a journal in labour or population economics.	21.74 (1.40)
Other fields	=1, if the study is published in a journal in other fields (economics or other sciences)	6.18 (0.81)
Adjusted number of citations	= Adjusted number of citations for the considered journal.	105124 (6099)
Social Science Impact Factor	= 5-years Social Science Citation Impact Factor.	14.96 (0.59)

Notes: binary dummy variables, with a value of 1 if condition is fulfilled and zero otherwise.

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